

## PUBLICATION POLICY AND THE *JOURNAL OF PARAPSYCHOLOGY*

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If we consider the published papers as being a journal's flesh and bones, and the editorial staff its heart and blood, then surely the publication policy is its will and conscience. None of the professional journals is without a publication policy, though probably only a minority of them explicitly state what it is. For many, it may be simply a tradition or a received opinion about what is appropriate for their audiences along with a method for selecting among competing submissions. In some cases, perhaps more than editors may care to admit, it is a single individual's or small group's preferences for what should be published.

Parapsychologists and, no doubt, other scientists who work in fields that might be described as being on the margins of mainstream science, are probably more aware of publication policies, implied or expressed, than most other scientists are. It is often their work, when submitted to mainstream journals, that challenges expressed publication policies, or forces hidden ones into the open. Of course, our own parapsychological journals have their publication policies, and in this paper I shall examine those of the *Journal of Parapsychology* as they have developed over the past 50 years.

The *JP* was born out of publication policy—the policy of *other* journals. When J. B. Rhine began his work in parapsychology, he did not experience any undue problems in getting his papers published. Several appeared in the *Journal of Abnormal and Social Psychology* and one in *Character and Personality*. But by the mid-1930s, when ESP began to achieve a measure of public interest and exposure, Rhine experienced a number of rejections. This suggested to him that parapsychological research might have difficulty in obtaining a fair hearing in the psychological journals.

By 1936, Rhine already had a small team of researchers at Duke and was in correspondence with several more around the country. Much of their research was publishable, and Rhine concluded that getting it all into the regular psychological journals might be a losing battle. Thus, in June of that year, he suggested to his friend and

confidant Gardner Murphy, "A regular periodical would do a great deal, I believe, to standardize work in the field, on the principle that nothing succeeds like success; that is, in the aspect of recognition. It would considerably stabilize and command respect for parapsychology to have a scientific journal in the hands of academic people in line with the best scientific publications and publishing only first quality experimental material."<sup>1</sup> Shortly thereafter, Rhine made a more or less formal proposal to William McDougall, Chairman of the Duke Psychology Department, that they should start regular publication of such a journal. McDougall, who was already experienced in founding journals (e.g., the *British Journal of Psychology*) readily agreed and helped arrange for the Duke University Press to publish it.

Rhine was not so persuasive with Gardner Murphy, who felt that parapsychology's results did not yet warrant a separate publication. Murphy seemed to favor continuing a system of mimeographed reports, reserving only the very best papers for submission to the regular psychological journals. In fact, after reading two of the papers destined for the first issue, he concluded that they were unsuitable for publication and asked to be excused from editorial involvement in the new journal.<sup>2</sup>

Under the joint editorship of McDougall and Rhine, the first quarterly issue of the *Journal of Parapsychology* appeared in March 1937. Naturally it opened with an editorial, penned by McDougall, that introduced the journal and, indeed, the new field of parapsychology. It was in these first pages of the first issue that the aims and policies of the editors were set forth. The "parapsychology" of the title came from the German and, according to McDougall (1937), "It may well be adopted into the English language to designate the more strictly experimental part of the whole field implied by psychical research as now pretty generally understood" (p. 7). He explained its implications for the policy of the *Journal*:

We do not claim that any sharp line can be drawn marking off the field of parapsychology within the larger vaguer province of psychical research. Rather, we anticipate that the stricter experimental methods will gradually invade other parts of the province annexing them to their own more special field, until possibly the two shall coincide. But we regard the differentiation of the two terms as useful at the present time;

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<sup>1</sup>Rhine to Murphy, 19 June 1936. (J. B. Rhine Papers, Manuscript Department, Perkins Library, Duke University, Durham, NC)

<sup>2</sup>Murphy to Rhine, 19 October 1936. (J. B. Rhine Papers, Manuscript Department, Perkins Library, Duke University, Durham, NC)

and it is our intention to admit to this journal only contributions that properly fall within the narrower sphere implied by its title; that is to say, reports of experimental studies in the stricter sense and discussions of methods and interpretations of such work. (p. 7)

Thus, McDougall indicated what the *Journal* would publish, and a few paragraphs later he indicated some of what it would not publish:

If, then, any of our readers should have trustworthy information concerning what may seem to be some sporadic display of mysterious powers or any other such phenomenon, intriguing but not susceptible of strictly experimental investigation, we shall be glad to hear of it; but we cannot undertake to publish any report of it. We beg him to send any such report to the Boston Society for Psychic Research, or to the London Society for Psychical Research. (p. 8)

What the *Journal* would publish, McDougall assured his readers, would be of the highest quality, even if it meant waiting for enough good material. But this, he felt, would not be an immediate problem, and he mentioned that repetitions of the Duke work were under way in many universities. Repetition was vitally important for parapsychology, argued McDougall, "first, to provide against the peculiarly great risks of *bona fide* error and self-deception; secondly, to carry conviction of the objectivity of reports and validity of conclusions to the naturally and properly skeptical public which this journal seeks to serve" (p. 8). As he concluded his editorial, McDougall promised:

We shall do our best to maintain a high critical standard, to guard against errors of all kinds, and to protect our readers against lack of good faith on the part of any contributor. But we shall not pretend to guarantee the accuracy of reports which we hope to publish, except those which may appear over our own signatures. (p. 8)

Thus was born the *Journal of Parapsychology*. It had the twin purposes of improving communication among scientists interested in the emerging field of parapsychology and of defining the boundaries and setting the reporting standards for this field. The selection of papers for the first issues was clearly meant to demonstrate the extent and the overall quality of the evidence for ESP. Some of the subsequent papers broadened the ESP research, and others began to address criticisms of earlier research. As Rhine (1957) himself later indicated, the most important task was to establish the case for ESP in a largely skeptical climate.

McDougall's optimism about the quantity of the material was short-lived. In October 1938, with subscriptions at about 600, Rhine complained about the lack of papers. In a letter to Gardner Murphy he expressed annoyance that Murphy was considering submitting a paper elsewhere, stating that "the *JP* needs good stuff badly."<sup>3</sup> Murphy replied with his previously expressed reservations about the *Journal*, but Rhine countered with an offer that took Murphy by surprise, "Do you want the *Journal*, lock, stock, and barrel?"<sup>4</sup> Murphy wrote back immediately asking clarification of the offer. In the ensuing correspondence over the next several weeks, Rhine and Murphy negotiated an acceptable transfer of control from Rhine and the Duke Lab to Murphy, Bernard Riess, and Ernest Taves.

But from the beginning, Murphy was not happy with the *JP*. He felt that the papers were too superficial to merit the serious attention of psychologists. Rhine, on the other hand, felt it was important to demonstrate the broad scope of research. Understandably, one of Murphy's chief concerns was whether he could shape policy along his own lines. Rhine replied, "The *Journal* is not committed except by custom to anything in the way of a set policy that cannot be easily changed."<sup>5</sup> Indeed, Rhine suggested that this would be an opportunity for Murphy to introduce the editorial policies that he had advocated. What finally persuaded Murphy to accept the offer was the hope that by doing so he could free Rhine to recreate his original experimental successes (Mauskopf & McVaugh, 1980).

For the new *JP*, Murphy decided to follow up on suggestions arising from the 1938 APA meeting and establish a review board of respected and more or less impartial psychologists. Murphy's introductory editorial (Murphy & Riess, 1939) set forth the new *JP* policy, noting that "emphasis is to be upon consistent technical reporting with very detailed accounts of experimental and statistical method" (p. 1). The editors would rely on the "Board of Review" for critical comments on submitted manuscripts. Toward the end of the editorial, Murphy again stressed his research philosophy: "It seems important, however, to insist that ESP research be intensive and thorough, rather than extensive and superficial" (p. 2). To embody this philosophy the new journal would publish one long monograph and several short articles, but the latter should be regarded as "suggestions in the direction of method."

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<sup>3</sup>Rhine to Murphy, 24 October 1938. (J. B. Rhine Papers, Manuscript Department, Perkins Library, Duke University, Durham, NC)

<sup>4</sup>Rhine to Murphy, 8 November 1938. (J. B. Rhine Papers, Manuscript Department, Perkins Library, Duke University, Durham, NC)

<sup>5</sup>Rhine to Murphy, 15 November 1938. (J. B. Rhine Papers, Manuscript Department, Perkins Library, Duke University, Durham, NC)

At first Murphy's hopes were realized. The Board of Review approved various submissions, and the *JP* published some of the most detailed experimental reports so far. Among the papers that passed the critical scrutiny of the Board of Review were the Pratt-Woodruff experiments, which were designed to address issues raised up to that time by psychologists. Mauskopf and McVaugh (1980) have noted that one of Murphy's editorial aims seemed to be to "normalize" ESP experiments by describing the experimental situation with enough precision to permit any other experimenter to repeat it.

As the war reduced the ranks of academics and the enthusiasm of the members of the Board of Review waned, it appeared by late 1941 that Murphy's editorial experiment had run its course. In the pages of the *Journal*, Rhine (1941) complained that the Board was in danger of failing in its purpose for lack of attention to the details of submitted papers.

It was no surprise, then, when in 1942 the *Journal* returned to Duke with J. B. Rhine as editor. Again, an editorial explained the change in editorship and introduced a "new program." Rhine explained that Murphy and Riess had asked to be relieved of their duties because of complications brought on by the war. Also, the management of the Board of Review had become too costly in terms of time and was yielding less and less in the way of improvement of the field. Accordingly, the editors would discontinue that system and replace it with a new section in the *Journal* entitled "Notes and Correspondence." Rhine (1942) invited all readers to comment on published papers, saying, "All comments by any reader should, we believe, be given careful attention and the mark of special authority reserved for none" (p. 2). The "Notes and Correspondence" section would be open for such comments and, where necessary, the original author's reply. Rhine noted, "The *Journal* has always welcomed criticism, and it is to be hoped that all *Journal* readers will accept our cordial invitation to participate in the discussion of the research reports as they are published" (p. 2). By using this new system, Rhine hoped to achieve the editorial benefits that he felt the Board of Review failed to provide. Viewing this policy with the advantage of hindsight, we can see that Rhine's invitation pioneered a level of editorial participation by readers not approached again until the 1970s when journals such as *Current Anthropology* and *Behavioral and Brain Sciences* introduced the concept of formal peer commentary as part of the publication.

That was not the only change in the new *JP*. Rhine planned to return the *Journal* to his philosophy of what would best advance the

field. From now on there would be a wider range of subject matter and, in addition to the technical reports, which remained the "backbone" of the *Journal*, there would be more general articles and reviews that would "presume less acquaintance with the special terminology of this field and with scientific and statistical method" (p. 3).

It was in this third incarnation that the *Journal of Parapsychology* hit its stride and established a pattern that endured for several decades. The battle for acceptance of parapsychology as a legitimate area of research had culminated in the publication of *Extrasensory Perception After Sixty Years* two years earlier, and the tide seemed to be running in favor of the parapsychologists. Free from the need to wage the battle alone, the *JP* could now settle down to reporting and communicating research to interested readers.

The *Journal* had already established a reputation for the quality of its papers, and that was, of course, a *sine qua non* for the *Journal* upon its return to Duke. What the *Journal* would do now would be to broaden the field of parapsychology by demonstrating the application of experimental methodology to other areas of psychical research, all the while attempting to strike a balance between technical reporting for the specialist and more general reporting to the interested nonspecialist. This last concern of Rhine's, that of keeping the research reported in the *Journal* accessible to the nonspecialist, remained with him throughout his association with the *Journal*.

But as Rhine (1977) was later to recall, the *JP* was facing its greatest test. The war had reduced his staff to two, and very little parapsychological research was being done anywhere. The chief concern at this time was to keep the *Journal* going and properly filled. The long-hoarded unpublished PK research was brought forward, both ensuring the survival of the *Journal* and carving out a major new territory for parapsychology.

In practice, the *Journal* settled into a routine that was to prevail for some years. Upon returning to Duke, it was edited by a triumvirate consisting of Rhine, C. E. Stuart, and J. G. Pratt, who were responsible for the decisions about what went into the *Journal*. Also, at this time it included a special statistical editor, whose function was to check for appropriateness and accuracy every paper containing statistics. Although the composition of the editorship changed somewhat over time (Betty Humphrey replaced Stuart, who died in 1947, and T. N. E. Greville joined J. A. Greenwood as a statistical editor), the basic supervisory structure of the *Journal* was set. These editors presided over an increasingly broad range of topics and diversity of presentation as the field of parapsychology grew.

As the *Journal's* horizons expanded, so did the editorial responsibilities. From the beginning, the editors had relied on an informal network of advisors who, though not referees in the modern sense, did assist in making decisions about the *Journal's* contents. In 1948, the editors formalized this somewhat by establishing an "Advisory Board" consisting of Hornell Hart, Professor of Sociology at Duke, Gardner Murphy, Professor H. H. Price of Oxford, and Dr. Robert H. Thouless of Cambridge. These men, an editorial acknowledged, were the more active of the *Journal's* informal "counsellors" (Rhine, 1948).

Thus, by the late 1940s, the mold for the *Journal of Parapsychology* was cast both in terms of its editorial structure and its content. It was run by a collective editorship augmented by the statistical editors and both formal and informal advisors. Its content defined and mirrored contemporary parapsychology and included not only experimental reports but also theoretical and review articles, book reviews, and, from time to time, news and comment. But it never strayed from its main role as the organ for experimental parapsychology. In general, the JP editorial policy remained stable for the next three decades until the present editor took over.

Whatever explicit and implicit policies were operative during those decades, there seems little disagreement about their being right for the times. They enabled parapsychology to grow and prosper in a way that probably would not have happened were it not for the *Journal's* role in communicating and focusing the research. But I do not wish to imply that there was no disagreement. Indeed, there was one aspect of editorial policy that did prove contentious, and eventually it provoked something of a cathartic crisis in the field.

The particular aspect of editorial policy that proved to be a problem was that of not publishing in full the experiments failing to show any significant evidence of psi. To be sure, the *Journal* always maintained a mechanism for making brief reports of nonsignificant findings—as early as 1938, it established a "Research Notes" section for chance results—but it did not publish in full any papers having only nonsignificant findings to report. By the late 1960s, some parapsychologists began to question the wisdom of this policy, and by the early 1970s there was enough grumbling in the field for Rhine to address the issue briefly in a 1972 comments section. In an article to answer the question "What's wrong with parapsychology now?" the sixth and final "complaint" was that "not publishing 'negative articles' (or chance results) is wrong." Rhine explained his belief that little can be learned from a report of an experiment that

failed to find psi, but he closed with a provocative comment, "However, most experimenters know well enough not to submit reports that communicate only the inadequacy of their approach, and no responsible editor wants to waste precious space on such papers" (Rhine, 1972, p. 85).

This remark did little to soothe the growing discontent among colleagues, but it did not immediately precipitate a crisis. That came later as a result of a longer comment section in 1975, the immediate cause of which was a submission of a paper to the *JP* and some correspondence in 1974.

In May of 1974 the *JP* received a paper reporting a careful replication of rodent ESP research that completely failed to find any evidence of psi. The paper was long and detailed, the editors considered the experiment well designed and executed, and the report was well presented. But there were no significant results, and after some further correspondence the editors were prepared to publish it only as an abstract. This was not acceptable to the authors, one of whom, in withdrawing the paper, replied to the *JP* editors that other parapsychologists had been following the progress of the paper and that they would be disappointed if the *JP* failed "to belie allegations . . . that it exercises unwarranted censorship of scientific results in refusing to publish so-called nonsignificant papers."<sup>6</sup>

To be sure, the authors of the paper, and indeed many of their colleagues, were unaware of certain aspects of *JP* policy when they raised the specter of censorship. For one thing, they believed that the policy of not publishing nonsignificant results was a covert one, and here they were clearly wrong. That policy had been announced in the *Journal* nearly 25 years earlier in an editorial entitled "Publication Policy." In this editorial in the March 1950 *Journal*, Rhine explained that, regarding chance results, "the editors feel that there is not space or reader interest enough to justify the publication of a report unless there is the possibility of learning something from such a failure. There are obviously too many ways of going wrong in the search for delicate capacities such as ESP and PK for us to draw any conclusions from a failure to obtain significant results" (p. 6).

Rhine's arguments for not publishing chance results essentially boiled down to two issues. One was properly an editorial one, whether enough could be learned from a chance report and

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<sup>6</sup>Broughton to Pope, 1 August 1974. (Archives, Foundation for Research on the Nature of Man, Durham, NC)



whether there was sufficient reader interest in such a report to justify taking up the limited space of the *Journal*. The other issue was a statistical one, in which Rhine maintained that each significant study "must rest on its own base" and that its evaluation should not be affected by others' failure to obtain results. While the editorial point could certainly be argued and defended, the statistical one was less defensible. Indeed, Rhine's friend and *JP* Editorial Advisor Gardner Murphy took strong exception to the statistical point. In a March 1950 letter, Murphy raised the counterposition that to properly evaluate a statistical conclusion of "significant," one must know the population of such studies from which it was drawn.<sup>7</sup> Murphy, who was reading a draft of the editorial, suggested that Rhine talk it over with his statistical editors, but he himself was not inclined to debate the issue in print.

Twenty-five years later, the issues were raised again. When the letter from the author of the nonsignificant paper arrived in August 1974, Rhine and the editors of the *JP* were occupied with more pressing matters. Levy's exposure had come only two months earlier and Rhine was busy doing all he could to minimize damage to the field. But he resolved to tackle the issue squarely when he discussed the matter with the authors of that paper during a visit later that month.

In the June 1975 issue of the *Journal*, the "Comments" section was devoted entirely to an article entitled "Publication Policy Regarding Nonsignificant Results." In it, Rhine argued the case for that policy, expanding on the same issues raised when the policy was first announced. On the statistical issue, he concluded that "the editors . . . have come to hold the position that so far as the statistical tests of significance are concerned one experiment is independent of another, and the results do not need to be pooled. In other words, a nonsignificant experimental result does not affect the significance of another one independently designed" (p. 136). On the issue of whether failures are necessarily instructive, Rhine argued that parapsychology was not at a sufficiently advanced stage in being able to control its experiments to permit us to learn much from failures. He urged experimenters to spend more time themselves trying to understand why the experiment failed by conducting further research rather than expecting limited journal space to be used to report the failure.

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<sup>7</sup>Murphy to Rhine, 3 March 1950. (J. B. Rhine Papers, Manuscript Department, Perkins Library, Duke University, Durham, NC)

Rhine addressed the issue of whether this policy amounted to censorship, though he did not use that word. He reminded readers that for much of its history the *JP* maintained sections in which chance results or failures to replicate were routinely reported in brief, and that the issue was not that of suppressing negative findings but of whether they merited full reporting.

As always, the pages of the *JP* were open to opposing opinions, of which there were many. In the March 1976 issue, three letters appeared. The first and longest was jointly from Dr. John Beloff, Richard Broughton, and Brian Millar, the latter two being the authors of the paper submitted in May of 1974, the paper that Rhine had used as an anonymous example in his editorial. In their letter they disputed the statistical point, arguing that the logical consequence of it was that "there is no longer any possible way of deciding even in principle whether [a successful experiment] was just the once-in-a-hundred lucky chance or whether the experiment had really hit upon a valid causal connection" (p. 89). They further contended that the *JP* policy ran grave risks of perpetuating artifacts or even encouraging fraud. The other letters, by Bierman and Eads, made similar points.

In the same issue, Rhine reiterated his arguments in favor of the policy and defended his position against the writers of the letters. This basically concluded the debate, which, for the most part, was not unlike the many debates in print that the *JP* had hosted over the years. One of the strengths of the *Journal's* editorial policy was its encouragement of public debate of contentious issues, as Dr. Palmer's accompanying paper in the present number of the *JP* will amply demonstrate. Certainly it was beneficial that the policy was clarified and removed from the suspicions that it amounted to a covert cover-up. The debate did not, however, convince many parapsychologists that the policy was still appropriate at this stage of the field's development.

There was some fall-out that went beyond the pages of the *JP*. The policy of not publishing nonsignificant results was one of the motivating factors behind the introduction of a new journal, the *European Journal of Parapsychology*, which began publishing in November 1975 and has gone on to become an important publication in the field. Possibly one of the more unfortunate aspects of the 1975 editorial was its timing, coming out as it did in the early summer of 1975. It was just in time to help polarize opinion in a dispute involving failed replications of the Levy work, work that had been submitted to the PA from the Institute for Parapsychology over

Rhine's objections. It was following this that the Council of the Parapsychological Association issued its statement "opposing any policy of discouraging the publication or public presentation of nonsignificant results or a policy of refusing to allow publication or dissemination of such results." This was duly printed in the December 1975 issue of the *JP*.

There is no doubt that this one aspect of editorial policy had little support in 1975. Perhaps it was justified at an earlier time, but certainly by the mid-70s it had become widely regarded as counter-productive. It must also be recognized that Rhine's determination to confront that particular issue head-on resulted in a healthier field of parapsychology and one more prepared to meet the challenges posed by its ever-expanding horizons.

That debate is, of course, history now. J. B. Rhine retired from *JP* activities in 1977. Dr. K. Ramakrishna Rao, as the new director of the Institute for Parapsychology, became one of the editors in that year, and he quietly moved the *Journal* to a more formal use of outside referees. To be sure, the *Journal* always had relied heavily on referees, but formerly they had usually been laboratory staff. Under Dr. Rao's guidance, the *Journal* editorial policy became one of reliance on the outcome of the peer review system to determine whether a paper merited publication, irrespective of the outcome of the statistical tests. By 1979, the *JP* began to list those individuals who had participated in the decision process at the end of each year.

It is well known that the peer review system for journals is not without problems. Several years ago, an innovative journal, *Behavioral and Brain Sciences*, initiated a lively debate on the merits and demerits of the process, which continues to this day (see, for example, Peters & Ceci, 1982). Nonetheless, it is generally regarded as the best we have, and it is with this system that the *Journal of Parapsychology* enters the second half of its first century.

As the *Journal of Parapsychology* crosses this threshold, we can see that the words with which J. B. Rhine closed his 1950 editorial on publication policy apply as much now as they did then, perhaps even more so:

We have had one important advantage in parapsychology, however, though it has not always been easy to appreciate it—we have been compelled at all times by vigorous criticism to look well to our methods. More conscious of our hazards than are workers in most of the sciences, we have developed an interest in the reliability of research methods that other inquirers have not needed to consider. We have also been made more than ordinarily conscious not only of research methods, but of

editorial problems as well, and we appreciate the importance of free channels for the outlet of advances in thought. Such experiences have given us some insights we should not otherwise have gained and some suggestions of value, we believe, even far beyond the boundaries of our research domain (p. 8).

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